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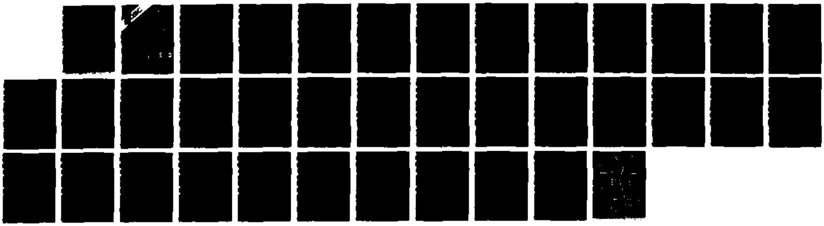

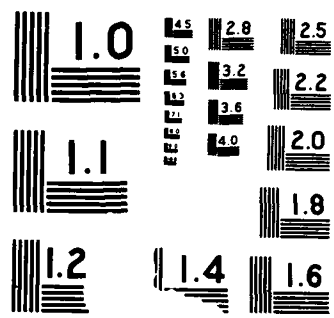
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Synoptic and Taxonomic Analysis of Form Perception Data and Theory

William R. Uttal

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During this program, the current state of form perception theory was reviewed, surveyed, and criticized, and the results were reported in a book written by William R. Uttal. That book consisted of six chapters, a preface, and an extensive bibliography; it will be published under the title "On Seeing Forms" in early 1988 by Lawrence Erlbaum Associates, Inc., Hillsdale, NJ 07642. An annotation of each chapter is provided in this report, as well as the entire concluding chapter that summarizes the lessons learned.

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ABSTRACT

This author reviewed, surveyed, and criticized the current state of form perception theory and reported the results of that synoptic review and critique in a book. The book that emerged from this review consisted of six chapters, a preface, and an extensive bibliography. This brief final report is presented as a summary of the fourth volume. It annotates each chapter and presents the final concluding chapter as a summary of what was learned during this review. The entire work is available from the publisher

Lawrence Erlbaum Associates, Inc.
365 Broadway
Hillsdale, New Jersey 07642

under the title "On Seeing Forms" by William R. Uttal in early 1988.

CONTENTS

1. OVERVIEW

The project summarized in this report had as its goal the production of a critical volume that reviewed and evaluated the current state of form perception theory in the final quarter of the 20th century. An extensive survey of the literature was carried out to determine what we knew concerning the fundamental question: How do we see forms? A broad, global, general answer to this question is that we still have only the barest glimmerings, in spite of a century of study, of what goes on within the human visual system. However, there has been great success in modeling and simulating human visual processes with cognitive, mathematical, computational, and neuroreductionistic approaches. An important conclusion of this work is that the latter is not equivalent to the former, i.e., models that only describe or simulate are not the same as a true and valid explanation of the inner logic of the organic visual system. This point is expanded substantially in Section 3. There is still a long road ahead to full understanding of this central aspect of cognitive function.

In the 666-page book resulting from this project, I suggested that we may, in fact, also not be on the right road. Heavily influenced by cellular neurophysiology and conventional computer technology, much of the contemporary research effort has gone into feature-oriented theoretical approaches. That a major shift to more global and holistically oriented empirical and theoretical research is desirable is a major conclusion.

The project, originally scheduled for 2 years, was completed in 21 months from 1 January 1986 to 30 September 1987. The book will be published by Lawrence Erlbaum Associates, Inc., Hillsdale, New Jersey, in 1988.

2. SUMMARY OF BOOK CHAPTERS

CHAPTER 1

The initial chapter provides a general orientation to the review, establishes the premises under which I proceeded, defines some of the critical terms, and spells out the issues, questions, and problems that are currently guiding form perception research.

CHAPTER 2

The second chapter is a review of form perception theories from classic times until the present. It organizes this kind of theory into two broad categories--the rationalist and the empiricist schools--and then suggests a modern rapprochement of these opposed points of view.

CHAPTER 3

The third chapter reviews theories of detection in four special topics in detection: (a) contrast detection; (b) dotted forms in dotted noise; (c) symmetry detection; and (d) masking.

CHAPTER 4

The fourth chapter provides some additional important definitions specific to the discrimination problem. It provides a discussion of four special topics in discrimination: (a) similarity; (b) texture discrimination; (c) matching; and (d) matching with transformations.

CHAPTER 5

The fifth chapter discusses the wide variety of form recognition theories. It deals with four special topics in recognition: (a) visual search; (b) object and word superiority; (c) computational reconstruction; and (d) individual form recognition.

CHAPTER 6

Chapter 6 contains the summary and conclusions of the project and is reproduced as lessons learned. It consists of the following sections:

A - Introduction
B - Some Sources of Difficulty
C - Some Theoretical Misjudgments
D - Do Laws of Perception Exist?
E - A Summary and a Prescription for the Future

3. LESSONS LEARNED: A CRITIQUE OF CONTEMPORARY APPROACHES TO FORM PERCEPTION THEORY

A. INTRODUCTION

This book has been written to review and evaluate the state of the psychological sciences in the 1980s with regard to our understanding of how people see forms. I have sought to develop a minitaxonomy of the various stages of visual information processing--detection, discrimination, and recognition--that I believe represents the core of the visual perception problem. These processes can generally be considered to be somewhat "higher level" than most of the visual processes discussed in my earlier works (Uttal, 1973, 1978, 1981), in that they are more likely than not to involve neural networks for their execution that are more central and more complex than the primarily transductive, communicative, or initial encoding mechanisms previously emphasized.

Somewhere within the sequence of processes that I have discussed here, there is a transition from what many of us now believe is a kind of more or less preattentive perception to a kind that is generally agreed to require effortful attention. It is my judgment based on a somewhat modest amount of evidence that this occurs at the interface between discrimination and recognition as I have specifically defined them in Chapter 1.

This book, as a conscious effort towards the development of some order in what otherwise often appears to be a disorganized collection of raw empirical and isolated microtheories, should be considered to be a continuation and culmination of the train of thought that has developed during the course of preparing this tetralogy. In this series of books I can see how my personal reductionistic approach has evolved from one stressing explanations that were patently and exclusively neurophysiological to one reflecting an equal commitment to psychological, descriptive, phenomenological, mathematical, and even philosophical interpretations. My perspective is now largely based upon the premise that perceptual processes may, in principle, be immune to a purely neuroreductionistic explanation. I suspect that it may, indeed, be beyond any reduction to process or mechanical implementation.

The outcomes of this evolution of personal theory, admittedly, may place me perilously far from the current consensus with regard to how we believe we see forms (a consensus that is dominated by direct, unmediated, algorithmic, elementalistic, empiricistic, and both mathematical and neuroreductionistic thinking), but I do see the dim outline of an emerging change in that consensus as reflected in the contemporary literature of perceptual theory. In an increasing number of instances, it seems to me that some of my colleagues are beginning to be concerned about today's Zeitgeist and are expressing an awareness that most interesting perceptual processes are not instantiated in simple and automatic mechanisms. There is a new realization of the enormous complexity of human perceptual processes and of the holistic, approximate, and indirect ways in which perceptual information is processed. The story has not yet fully unfolded; there are many conceptual and technical barriers yet to be overcome, but there are a few theorists (e.g., Teller, 1984) who are also currently expressing these new points of view.

One of the disappointing developments associated with this new enlightenment, however valid it may be, is that we are now beginning to see some signs of limits or constraints on what we may hope to accomplish even in the ultimate future. These limits may be fundamental and may inhibit us from achieving some of the more or less naive goals set by cognitive, computational, and physiological psychologies in recent years. Upon reflection, the hubris that characterizes some of the goals set by students of perception for themselves is astonishing. That hubris is often compounded by a lack of awareness of the constraints imposed on theory building in this field by mathematical and logical principles that are much more directly applicable to perceptual research than many perceptual psychologists and other cognitive psychologists yet acknowledge.

In pursuing this project I have sought to step outside the system sufficiently to at least permit myself to maintain the pretense that I am not championing a vested theoretical interest or grinding a personal ax. Not having been a top-down, cognitively oriented psychologist in the past has helped me to maintain that personal illusion. From this somewhat detached position I have assumed the roles of gadfly and critic. My hope is that this book, which has turned out to be much more of a critique of the field of perceptual theory than the synoptic review I had originally intended, is neither unfair nor abrasive, but constructive and positive. I remain a true believer in

this science and am convinced that even those approaches of explanation I have criticized can be either beautiful or useful in their own right--or both. Whatever I say in this chapter about the validity of neurophysiology, artificial intelligence, or mathematics as models of form perception should not be misinterpreted to mean that I think these fields of endeavor should be abandoned. They have interest, utility, elegance, and beauty sufficient to stand on their own and do not require perceptual theory to be sustained. I am only concerned here with the linkages that are made between them and the *truth* about perception.

Having assumed the role of a critic, it would be unfair for me not to further acknowledge at the outset that the current state of theoretical affairs in perceptual psychology may be as good as it can possibly be, given the intellectual forces acting on it--forces that come both from the nature of the material and the quality of the tools available from other closely related sciences. Some of the powerful intellectuals who have contributed to this field may be incorrect in their interpretations from my point of view, but neither I nor anyone else could have reacted to these invalid (in my judgment) interpretations without the pioneering contributions of those colleagues and predecessors. It is their thoughts, right or wrong, that represent the foundation on which future understanding will be built. Sometimes progress is enhanced because of a mistake or misinterpretation that highlights some conceptual confusion.

My goal has been to concentrate on form perception theory in this book. I now believe that it would have been both impossible and wasteful to try to review all of the empirical evidence for all related perceptual phenomena. Theory is, first of all, why we are at work--theory in the most positive sense of integrative understanding and explanation. Theory is an important digester and summarizer of what sometimes may seem to be a tangle of trivial and irrelevant experiments. It is a truism, but a necessary one to assert, that it is only to the extent that an experiment contributes to integrative theory that it is worthwhile.

B. SOME SOURCES OF DIFFICULTY

When I began this book it was with a deep fascination with the wondrous accomplishments of perceptual theory. At the conclusion of this project I find myself much less sanguine about the state of integrative thought in this field for several reasons. Each of these can be traced to a particular source.

1. Much of what passes for explanatory theory these days has been misdirected by the accidents of what statistical, mathematical, or computational tools are fortuitously available. In particular, there is an enormous gap between the kind of mathematical processes that are necessary to describe the logic of the human visual system and currently available formal systems. Some tools (e.g., Fourier transformations) have quite correctly gained wide acceptance as convenient nomenclatural or descriptive systems, but quite incorrectly have been extended to incorporate superfluous and interpretive pseudoreductionistic baggage that could easily be and should better have been kept separate.

2. It seems to me as a result of my studies that there has been a paucity of thinking about fundamentals. What is it that we are trying to do? What do other sciences and methodologies such as combinatorial mathematics, chaos theory, and automata theory (to highlight only three particularly salient examples) have to say about the strategies with which such goals can be approached. To assert that such matters are just "philosophical" (a word read by many as being synonymous with "frivolous") excursions misses the point that formal logic, mathematics, and epistemology have always acted and must continue to act as guides and constraints to speculation concerning the study of mental processes just as the laws of physics have constrained what might be ill-posed efforts in applied engineering. Most practitioners of the physical arts and sciences know that it would be a wasted effort to attempt to do something (e.g., develop a perpetual-motion machine or build a faster-than-light space ship) that is formally excluded by those laws. Sometimes, however, it seems that many of my perceptual colleagues attempt to achieve goals that should have been similarly excluded by analogous constraints and fundamental principles. Sometimes this is due to the fact that they are not familiar with the caveats of mathematics or physics. Sometimes it seems that it is because they ignore these principles even though they are familiar with them. Frankly, I am at a loss to explain these logical lacunas.

There are important limits and constraints that must be attended to by perceptual theoreticians. One of the most important is the mathematical fact expressed by Stockmeyer and Chandra (1979) that "some kinds of combinatorial problems require for their solution a computer as large as the universe running for at least as long as the age of the universe" in spite of the fact that they are not infinite and are "solvable in principle." The germane fact for perceptual and other kinds of reductionistically oriented cognitive scientists is that such "intractable problems" include systems as simple as a checkerboard (and the rules of checkers or chess). It seems that given the intractability of this simple problem it would be well for us to consider the discrepancy between the magnitude of the connectivity of the checkerboard and that of the brain before we somewhat overambitiously attempt to analyze the cognitive functions of the brain from the same elemental level.

There are several misperceptions at work here. One is that if a computer program simulates to some degree some perceptual process, it is akin to an explanation of that process. In fact, such a simulation is virtually always at the level of the molar *process* or *function* of the simulated system; it is an extrapolation of the most fundamental tenet of analog theory that the instantiated mechanism by which a given algorithm works is usually completely different than the mechanism by which the brain works and yet both may be explained by the same formulae. For another, consider the relationship between the measurement of pressure in a container and the properties of the ensemble of the gaseous molecules within that container. The overall measure--pressure--though directly related to the concatenation of the microscopic details of molecular position and dynamics tells us nothing about the behavior of the molecules. There is no way to go backwards from the global to the locally discrete. Yet, reductionistically oriented psychologists quite commonly assert that we can adduce something about the behavior of individual neurons from the molar phenomenology of the perceiver. On the other side of the

coin, although it is possible in principle, it is in practice very difficult to go from the discrete to the global even when the number of elements is not very large. A bit of thinking about fundamentals seems to be in order here.

Considering the number of neurons the combinatorics of the human brain become formidable indeed. Although there is no universally agreed measure of complexity, one can get a feeling for the magnitude of the problem by appreciating that there are something like 10^{10} (or, as some would assert, 10^{13} if all of the very small cells of the cerebellum are counted) neurons and that each neuron may be connected to as many as 10^3 other neurons. Considering that comparably huge numbers of cerebral neurons may be involved in even the most simple percept, thought, or cognition, but without the regularity that permitted some early stabs at network simulations (e.g., in the retina or cerebellum), this is all too likely to be an intractable problem in the way identified by Stockmeyer and Chandra (1979). This is some checkerboard, indeed!

The perceptual theorizer is often misled by an uncritical acceptance of the degree of accomplishment of simple network simulations of some perceptual process. In fact, no network model has ever produced behavior of the complexity of the simplest organism with, perhaps, only one area of exception--those situations in which the outcome is due to the function of highly repetitive, almost quasi-crystalline kinds of structures such as has occurred in the modeling of the Mach band by lateral inhibitory interactions in the retina and some interesting analyses of the equally highly regular cerebellar network. But the irregular network of even a small portion of the central nervous system of a vertebrate has never been analyzed, much less simulated in a computer program. Models that purport to do so are often very simple networks of a few cells that analogize simple forms of molar behavior. Thus, simple networks of a few neurons (such as those found in invertebrate nervous systems) can be used to produce behavior that is comparable to that produced by a much more complicated vertebrate organism. Here, too, there is ample opportunity for a fallacious leap from the minimal model to the fullblown network of the more complicated brain because of analogous behavior when, in fact, the actual processes and mechanisms in model and organism are probably totally different.

3. A third cause of my concern about the state of integrative thought in this field is that perceptual psychologists seem to be all too easily attracted to distinguished accomplishments in other sciences as putative models even when those data are demonstrably invalid in the context of cognition and mental processes. It is an oft-told aphorism that experimental psychologists really want to be physicists. To the extent that this means that they should introduce quantitative procedures and precise measurements into well-controlled experiments, this is all to the good. To the extent that they assume that the same kind of simple, monodimensional, functional relationships that characterize the study of physical entities are characteristic of what is intrinsically a multidimensional perceptual universe, this is probably a mistake.

Many other psychologists seem to have an inexorable desire to be neurophysiologists and to study neurons rather than mental phenomena. While the study of individual neurons is exciting in its own right and has proven to be a seductive heuristic for perceptual theory, it is important to appreciate that the outstanding intellectual achievements that have resulted from the invention of the microelectrode by Ling and Gerard (1949) may be totally irrelevant to perception or cognition. Most of what we have learned from this powerful tool deals with the metabolism and function of *individual* neurons. We also have a good idea of the function of isolated synaptic junctions between pairs of neurons, but we have very limited knowledge of the nature of the realistically complicated networks into which they are connected. Nevertheless, this missing information is the only kind that is really relevant to developing a sound neuroreductionistic theory of perceptual processes. It seems far more likely that perception is the outcome of the *interaction* of myriads of neurons and not the *action* of a single cell. In spite of this clash between levels of discourse and a substantial amount of data to the contrary, single-cell hypotheses purporting to explain perceptual processes still hold wide sway among perceptual theorists.

Similarly, many psychologists seem to want to be computer scientists in contemporary science. Although the computer is the closest analog to the brain our technology has yet produced, these two distinctly different kinds of information processors are almost certainly organized internally in accord with fundamentally different principles and operate according to totally different rules and logics. Nevertheless, the computer metaphor of perceptual processes is also widely accepted, particularly among cognitive psychologists, with what seems to be a minimum of critical examination of the fundamental issues raised and without careful attention to the formal rules that constrain achievement of their often ill-defined goals. In retrospect, it seems that we have been all too willing to let the conveniences of the computer model distract us from the principles of perceptual organization that are clearly expressed in observed phenomenology.

4. Another concern is that perceptual psychologists are all too prone to inadequately define the entities and concepts that they study. We all acknowledge how intrinsically difficult it is to define mental terms; even such basic operational terms as *detection* or *discrimination* are used in a variety of different and often conflicting ways in various experimental reports. For example, we have seen in Chapter 5 how the word discrimination (in the context of visual search) is often used to describe what is clearly a recognition process. In other instances "discrimination" has been used to define the task in what clearly seem to be detection experiments. The lack of precise operational definitions of many of the most fundamental ideas of perceptual psychology often leads to a situation in which contradictory findings are ignored or false controversies engendered.

A further definitional difficulty is often produced when the same word is used to denote two quite different processes that share some superficial functional similarities. Is, for example, the dishabituation of the response to a touch to the mantle observed in the aplysia a reflection of the same kind of neural mechanism as that underlying human adaptation to seeing the same

movie three of four times? Are we using words that are merely metaphors or analogs of each other to incorrectly identify distinctly different structural entities as homologs of each other?

5. Perceptual theorists also tend to both ignore the first-order demonstrations that should set the context and limit the data base to which they apply their models. In a previous chapter, I discussed the role of demonstrations and how important they are to establishing the basic premises (e.g., recognition is locally or globally precedent) of the intellectual domain of perceptual theory even when they are not sufficient by themselves to authenticate a perceptual theory (see following). All too often the message being broadcast by these important pieces of first-order data is ignored. It seems to me, for example, that many of these demonstrations, if not most, argue that we should pursue a global, interpretive theoretical approach. To the contrary, however, much of contemporary perceptual theory seems to be more influenced by the available computational technology than by these phenomena in its initial choice of the strategy by means of which a theoretical attack will be made. Not unexpectedly, artificial controversies sometime occur between theories that are based on what appear to be totally different premises concerning the nature of psychobiological reality. But even worse, theoretical traditions sometimes tend to perpetuate themselves and to be reinforced by analogies and metaphors even though they are not speaking to the true mechanisms by means of which perceptual processes are carried out.

6. Further, psychologists, particularly those involved in the development of theories of vision, have often not heeded the empirical facts of modern cognitive psychology. They still tend to assume that passive and automatic processes account for most perceptual phenomena when, in fact, there is an emerging feeling that perception is far more a matter of interpretation and active reconstruction than of "hard-wired," automatic, and algorithmic calculations. Bruce Bridgeman (1987) summed it up well in the introduction to a recent book review when he asserted:

> A specter is haunting neuroscience. It is the specter of cognition, of higher level influences that can no longer be ignored.
>
> (Page 373)

Bridgeman goes on to assert this is "motivating systematic attempts at interaction between neurobiologists and cognitive psychologists." But there is another possibility, namely, that active, cognitive processes cannot be modeled by simulations of neural networks for the reasons I have considered earlier and will summarize later in this chapter. Perhaps the message of modern cognitive psychology's empirical research program--that perception is far more complicated than it previously seemed and higher level effects penetrate down far lower than we had thought--is also being ignored.

7. Another cause for concern is that very grand perceptual theories are often based on astonishingly slight amounts of data: A single type of experimental result or even the outcome of a single experiment or demonstration has often been used to justify the most sweeping generalizations concerning how people see forms. Given the very low level of validity testing,

virtually any microtheory can be supported simply because there are always some equally microscopic phenomena that can be made to appear to be relevant. However, because of the enormous variability of human perceptual and cognitive skills, it is often the case that vastly different conclusions can emerge from what are thought to be nearly identical experiments. Fit with a single experimental datum should not be allowed to validate any theory. It is easy to find a formula to describe individual phenomena; but theories of worth should describe broad classes of data.

I have criticized the artificial intelligence movement for developing programs that are not generalizable and, thus, cannot be considered to be intelligent in the sense applied to organic reasoning. But psychologists should also be aware that many of our theories also do not generalize beyond the immediate empirical environment in which they were created. Those that do not are terribly vulnerable to repudiation.

8. Further, there is a desperate shortage of mathematical techniques and algorithms that are appropriate for the study of complex, interacting systems in which the global structure or arrangement of parts is more important than the nature of the local features. Perceptual and other kinds of cognitive psychology are very much in need of something that can play a role analogous to that played by the noneuclidean mathematics of Lobachevsky and Riemann in relativistic physics. Those mathematical developments allowed physics to take its giant leap forward early in this century. We might not generally recognize it when it comes along, but hopefully someone will perform the essential Einsteinean task of linking a truly applicable and relevant mathematics to a truly valid theory of perceptual and other cognitive phenomena. Bennett, Hoffman, and Prakesh's (1986) *Observer Theory* is a recent attempt to provide such a mathematical foundation, but the mathematics is extremely difficult, the conceptual structure is subtle and cryptic, and it may be some time before this work becomes accessible to and understandable by other perceptual theorists.

One of the most glaring lacuna in this regard must be filled by some novel mathematics that will help us to understand how the molar phenomenological properties of perception can emerge from the concatenated action of a host of discrete neurons. Though there are very few insights in the way in which this may occur, one possible line of thought has been stimulated by Hoffman's (1966, 1980) application of the Lie algebra to perception (see Chapter 2). But there are also some other obvious leads that must be followed up. It is, of course, possible that there will never be a completely satisfactory mathematics (mathematics as we know it may not be adequate to model mind), but until we can establish such a dismal fact beyond a reasonable doubt it would be foolhardy to abort our efforts in that direction.

Neurobiological networks, of course, do produce intelligent behavior (any intellectual accomplishment and any cognitive process stands as an existence proof of that fundamental truth), but there is a great need for research and development of more appropriate kinds of mathematics to represent the particular kind of computational engine--the brain--with which we are concerned. The necessity for establishing this foundation of understanding of how neural

networks compute and the logic they use is very great. We must not deceive ourselves, however; this task will certainly be very difficult and may be impossible. Along with this search must go a substantial effort to determine what are the limits of analysis of such problems.

9. The final source of my concern is that the very empirical data upon which we perceptionists base our theories are often embarrassingly transient. We have seen throughout this book many instances in which some observation has drifted away like a smoke ring when attempts are made at replication. I argue that this paucity of reliable data is, at the most fundamental level, a result of the multivariate complexity of most psychological functions, and the adaptive power of the cognitive system rather than any deficiency of control in a given experimental design. Nevertheless, the fragility of even the data base upon which we base our theories is a signal of the extreme care that must be taken in such complex situations to guarantee data validity.

In short, because of the plasticity and adaptability of the organic mind-brain, perceptual research is a far more treacherous universe for the theoretician or empirical researcher than the much simpler worlds of basic particle or cosmological physics. Later in this concluding chapter I shall consider the widespread problem of the enormous changes in findings that can result from the slightest changes in experimental design, as well as the surprising absence of general rules able to describe reasonably broad ranges of psychological phenomena.

C. SOME THEORETICAL MISJUDGMENTS

What are the misjudgments that we have collectively made with regard to current perceptual theory as a result of these forces?

1. My discussion of the major misjudgments begins with the fact that there has been much too great an emphasis on local-feature-oriented models of perception rather than theories accentuating the global, gestalt, holistic attributes of stimulus-forms. In my judgment, all too many perceptual theorists have currently fallen victim to the elementalist technological Zeitgeist established by neurophysiology and computer science. Although disappointing, this is not surprising, because it is exactly comparable to the way in which theoreticians in this field have been influenced by the pneumatic, hydraulic, horological, and telephonic analogies that have sequentially characterized theories of perception and mind over the last two millennia.

In spite of this fallacious tendency toward features, a considerable body of evidence, only some of which has been surveyed in this volume, suggests that in fact we see holistically, that is, that there is a primary global precedence in human perception. We can, of course, direct our attention and scrutiny to the details of a picture, but we are as often ready to perceptually create details on the basis of some inference as we are to be influenced by the presence of the real physical details of the stimulus. First-order demonstrations, usually ignored but always compelling, urge us to consider the global aspects of a form, whereas one has to carefully construct an experimental situation to tease out some semblance of local precedence. In

retrospect, we can now appreciate that classic Gestalt psychologists set a tone that was fundamentally correct in spite of the fact that they were no better at explaining phenomena than we are today; and many well-controlled contemporary experimental studies also suggest a global precedence in our perception of the world around us. We have not listened to these messages.

2. Further, there has been much too much emphasis on theories that are essentially empiricist rather than rationalist. We tend to develop models of recognition, in particular, that depend upon a passive transformation of the geometry of the stimulus-form by algorithms that are supposed to be directly sensitive to the details of the local geometry of the stimulus.

In fact, it seems far more likely that the level of cognitive penetrability of perceptual processes is far greater than is generally accepted today within the empiricist tradition. What I am suggesting here is that, in psychophysical reality, there is a much more adaptive and interpretive processing of the symbolic content of the coded stimulus information transmitted along the peripheral sensory pathways than is generally appreciated. These codes, the media for the communicated information, are not the essential input for some kind of passive processing, but rather are only cues and hints that are used by more complex and higher level (i.e., more central) systems that very quickly throw out the specifics of the stimulus geometry. The meaningful messages--the relationships and salient relevancies--conveyed by those "media"--the neural signals and codes--are then processed in what can best be called a symbolic manner that has little to do with the physics or geometry of the stimulus or the dimensions of the neural codes by which the information was transmitted to the central nervous system.

A corollary of this misinterpretation of perceptual reality, in which a valid rationalism has been supplanted by a superficially easy but spurious empiricism, is evidenced in the many models (of what are almost certainly very complicated, high-level perceptual processes) that are erroneously based on known peripheral neural mechanisms. This tendency to descend to the periphery for explanations is a miscalculation of the same order as the tendency towards deterministic, passive process models--in other words those that I have classified as empiricistic. At some later time, I am convinced, we shall come to agree that more symbolically representative and computationally active processes--those that I have classified as rationalistic--are actually at work here.

In short, there is an ubiquitous fuzziness in the selection of the point of demarcation between the strictly deterministic, passive, and preattentive processes of the peripheral nervous system, on the one hand, and the active, symbol-processing, attentive visual processes on the other. It appears that cognitive penetration can be discerned in recognition behavior but much less so in the lower levels of visual processing. My own research on the perception of dotted, stereoscopic forms, as well as many conversations with others who also work with stereoscopic stimuli, suggest to me that the cognitive penetration of which I speak is especially salient in studies involving actual three-dimensional stimuli or equivalent virtual objects inferred from two-dimensional invariances.

3. Another misjudgment has been a profound unwillingness to accept the seemingly obvious fact that perceptual processes are immensely more complex in terms of the numbers of involved variables, neurons, mechanisms, and processes than hitherto believed. A corollary of this misunderstanding is the general lack of appreciation of how sensitive most perceptual data are to relatively trivial changes in experimental design. To put it most directly, we have neither appreciated how complex even the simplest appearing perceptual process actually is nor how adaptive the human is.

4. For another misjudgment we perceptionists exhibit a bizarre tendency to become involved in controversies between nonexclusive alternatives or dichotomies that actually represent only the end points of a continuum. As is typical of all of the sciences, usually both extreme points of view have some support and eventually both are reconciled in terms of some compromise theory.

5. And another: The descriptive powers of mathematical models are often not distinguished from the many alternative physiological or physical instantiations that can be modeled by any single one of these formularizations. The possibility of analogous processes, describable with a common mathematics, but implemented by totally nonhomologous mechanisms is all too often ignored in perceptual theory.

6. Further, we tend not to be appreciative of the constraints and limits imposed upon the analysis of internal structure by behavioral methods. Moore's (1956) second theorem, the "black box" limit from engineering, and simple combinatorics all argue that in principle some of the goals of perceptual theorists, particularly those of the "cognitive" persuasion in which attempts are made to determine the inner mechanisms or processes underlying some observed or inferred mental states, are spurious and overly ambitious.

It is worthwhile to restate Moore's (1956) theorem, which is so central to this argument.

> Given any machine S and any multiple experiment performed on S, there exist other machines experimentally distinguishable from S for which the original experiments would have had the same outcome.
>
> (p. 140)

In other words, no experimental tests of a completely unknown and closed automaton can by themselves ever distinguish between two alternative hypotheses that both adequately predict or explain the internal workings of the machine.

There are, indisputably, many ways in which to challenge the relevancy of Moore's theorem to perceptual science: Is the brain an automaton in the strict sense of the term? Are there other ways to restrict the number of alternatives and thus to converge on a valid understanding of

inner mechanisms? Is the theorem even true? However, in the absence of proven alternatives and equally formal disproofs, it seems appropriate to accept this proof and to acknowledge the fact that there may be, *in principle*, limits on how far we can go in searching for a structural explanation or analysis of the kind of closed and complex system exemplified by the human mind-brain.

To make this point clear, let me suggest that although it is very easy to specify that 2 + 2 = 4, it is impossible, given only the outcome of some such calculation (e.g., 4) to establish what the algorithm or process was that led to that particular number. It might have been 5-1, 3 + 1, 2^2, or any of an infinite number of computational pathways that led to a 4. Even though it is possible to eliminate certain hypotheses, especially in a simple numeric system such as the one exemplified here, there will always be a very large number, perhaps an infinite number, of possible alternatives remaining.

A closely related argument expressing the difficulty of understanding complex systems even when they are open to internal examination has been made by Crutchfield, Farmer, Packard, and Shaw (1986) in their important and clear discussion of chaos theory--an important new development in mathematics. These authors point out that seemingly random behavior can be generated by the concatenation of very simple deterministic systems.[1] The apparent "chaos" of a plume of smoke, for example, unanalyzable in terms of its origins, results from the amplification of small uncertainties by even very simple interactions in systems of many constituent components and repeated processing steps.

If one interprets random states as being, by definition, irreducible to regular, periodic, or lawful forcing functions, then the implication of this new development is that even though simple rules and processes may be involved at some primitive elemental level, it may be impossible to go backwards from the resulting chaotic or random state (for example, the details of the neural activity of the brain underlying some mental act) to understand the steps by means of which the outcome was achieved. This is a handicap added to the difficulty of going from the answer 4 to the problem that generated that particular number. 4 is not reversible to 2 + 2 because there are so many routes along which one can pass to get to 4. Chaos is not reversible no matter how precise our knowledge of the details of the processes that led to it, because the paths have been obscured by the "magnification of small uncertainties." If both correct and applicable, this theorem essentially negates the top-down approach of cognitive oriented psychologists.

Similarly, the goal of realistically synthesizing complex behavior from the bottom up (by means of neural net models) is also challenged. If concatenations of even modest numbers of real or modeled neurons quickly produce apparently chaotic behavior, we will never be able to distinguish one of those neural chaoses from another; two "random" systems of this kind would

[1]Though Crutchfield and his colleagues assert that this is a "striking" and novel development, the same point was made by Cox and Smith (1954) over 30 years ago and was well known among some statisticians.

look very much alike (in that both are random) even if they are actually performing very different perceptual functions. Thus, *brain* (meaning a complex neural network) states would be indiscriminable, regardless of how well the mental states can be distinguished. Brain-mind associations at the network level would, therefore, be impossible.

Crutchfield and his colleagues (1986) go on to explicitly make the same point I wish to make here. According to them, chaos "implies new fundamental limits on the ability to make predictions." I assert that, in addition, chaos theory also implies new and fundamental limits on our ability to disassemble complex behavior into its neural cellular constituents. Although these authors go on to assert that some classes of random processes may actually be opened to analysis using the mathematics of chaos theory, their argument mainly can be read as implying that there will be fundamental limits emerging from this analysis that will constrain our ability to understand the breakup of smoke, the weather, the erratics of fluid motion, or the operation of a very large number of neurons in some perceptual process. In all of these cases, the strong implication is that it is unlikely that we will ever be able to go backward or down from the concatenated outcome to the rules of the individual elements. In each of these cases, the output is so complicated that it must essentially be considered to be random--thus precluding reductionistic analysis. Although it is possible to demonstrate some global measure that may satisfactorily summarize these random states in some statistical manner (e.g., pressure, the ratio of turbulent to laminar flow, or--perceptual phenomenology), reduction to the behavior of individual elements is impossible *in principle as well as in practice* according to chaos theory!

In short, the important conclusion is that where such a constraint had previously been considered to be one of simple complexity and *in practice* limits on computability, chaos theory now suggests that there may be *in principle* limits. These systems are not just complex and multivariate, they produce random--that is, intrinsically unanalyzable--behavior.

Crutchfield, Farmer, Packard, and Shaw (1986) also note that extreme numerosity of interacting components is not necessary for a chaotic situation to occur--chaos can arise out of the uncertainty that is inherent in repetitive processes of interaction among a few components as well as a result of the number of components. They allude to such "simple" systems as the collision of a few billiard balls as potentially evidencing chaotic behavior in very short periods of time. The reason is that at the microscopic level, even the billiard table and balls turn out not to be a simple system--at each collision there is enormous uncertainty about the points of curvature at which the balls will collide and as each ball travels across the table, it is undergoing serial interactions each of which inserts its own microscopic portion of uncertainty into the quickly emerging chaotic behavior of the balls.

Crutchfield, Farmer, Packard, and Shaw's discussion of chaos is not just mathematical esoterica--it is directly relevant and specifically damning to the neuroreductionistic strategies and theories that I have challenged here. This relevance can be made most clear by letting them speak for themselves, as follows:

Chaos brings a new challenge to the reductionist view that a system can be understood by breaking it down and studying each piece. This view has been prevalent in science in part because there are so many systems for which the behavior of the whole is the sum of its parts. Chaos demonstrates, however, that a system can have complicated behavior that emerges as a consequence of simple, nonlinear interaction of only a few components.

The problem is becoming acute in a wide range of scientific disciplines, from describing microscopic physics to modeling macroscopic behavior of biological organisms. The ability to obtain detailed knowledge of a system's structure has undergone a tremendous advance in recent years, but the ability to integrate this knowledge has been stymied by the lack of a proper conceptual framework within which to describe qualitative behavior. *For example, even with a complete map of the nervous system of a simple organism, such as the nematode studied by Sidney Brenner of the University of Cambridge, the organism's behavior can not be deduced.* Similarly, the hope that physics could be complete with an increasingly detailed understanding of fundamental physical forces and constituents is unfounded. *The interaction of components on one scale can lead to complex global behavior on a larger scale that in general cannot be deduced from knowledge of the individual components.* (Italics added)

(p. 56)

7. Another misjudgment is that perceptual theorists display a terrible weakness for deifying the concepts that they originally invoked merely as useful heuristics or metaphors with which to think about some of these terribly difficult problems or as computational analogs with which to describe them. Just as we have progressed to a point in our scientific knowledge of brain and behavior at which no one should now think that there is a homunculus in our head, neither should any reasonable student of cognition now accept the existence of any "list processor" or anything like an "expert system" between our ears. Furthermore, based on what we know about the limits of the neuroreductionistic strategy, perhaps not even the apparent successes of the simplistic, quasicrystalline type of nerve net models (the regularity of which is their sine qua non) should be allowed to influence our thinking about perceptual systems. The tools of the simulation trade are not a priori good theories of how the mind works, and to go backwards from even excellent imitations of real cognition to detailed, unique conclusions concerning internal structure and logic in the perceiving brain is patently absurd.

8. Finally, there is a pervasive tendency throughout our profession *not* to accept three fundamental facts concerning that which modern science has *not* yet accomplished.

a. First, we still have not the slightest inkling of how we bridge the gap from the action of discrete neurons to the molar mental processes that are indisputably the outcome of the interaction of vast networks of these same neurons. Our discussion of chaos theory and simple combinatorics suggest that such a goal may not be ultimately achievable; even given that any conclusion about the state of the science in the distant future may be arguable, certainly we have not yet achieved any such bridging hypothesis or explanation in our current psychobiological science.

b. Second, we still have virtually no information about the kind of logic and logical processes that are executed by the brain in carrying out perceptual processes at more molar levels. The best nonneural, process-oriented analogies, those proposed by modern computational vision experts, have no better justification than some of the other more primitive metaphors proposed as verbal or statistical models by earlier cognitive psychologists. The computational models, as David Marr (1982)--the late founding father of the field--so correctly asserted, only describe and simulate the transformations that "must" be made to go from an input to an output; but they, like all mathematical models, are essentially indeterminate with regard to the particular neural mechanism by which each step in the transformation is made. To assume that we can take a pair of two-dimensional images and extract invariant information concerning depth from them is a totally reasonable behavioral description of a visual process. To assume that contour enhancement takes place somewhere within this process is a plausible hypothesis. However, to specifically assume that there is a Laplacian operator in the brain is an unwarranted extrapolation from those reasonable assumptions. The point is that even the best fitting mathematical description is not tantamount to a unique definition of internal mechanisms. There are many different sequences of processes that will lead to the same transformation between input and output.

c. Third in this same vein, my retrospective examination of the discussions in earlier chapters of this book suggests that there is currently precious little data to vigorously support any "cognitive-type" model of internal process. Controversy, disagreement, and conflict seem to be more characteristic of what we think we know about internal processes than is consensus. This ongoing inability to achieve closure in cognitive studies may itself once again reflect the fact, as I argue, that the exposition of internal mechanisms by behavioral techniques may be in *principle* an unsolvable problem.

I must stress that it is totally inappropriate to consider any of the major theoretical transgressions in perceptual theory as the product of an "incompetent" group of perceptual scientists. This is not the case. The real source of these difficulties with contemporary theory is the terrible task that psychologists, in general, and perceptual theoreticians, in particular, have set for themselves. We are not dealing with simple, single-valued functions, but with an active, adaptive, interpreting, responding, self-modifying mechanism--the human brain--that often changes the rules of the perceptual game in midstream. How often an experiment has "failed" because the observers "played a different game" than the one the experimenter tried to define by

the experimental protocol? How varied is our repertoire of illusions[2]--the common discrepancies between the message of the physical stimulus and the perceptual responses? The perceiving brain does not slavishly respond to stimuli but *acts* upon them. While the experimenter tries hard to devise a clever task to get at an underlying process, the observer in the experiment is working equally hard to devise a clever process to adapt to the needs of the task.

Thus, cognitive penetration seems to reach deep into the visual process; into processes that at first glance seem to happen almost automatically, but which, in fact, on close analysis reflect more of Helmholtz's unconscious inference than it is popular to admit these days. The "cognitive specter," to which Bridgeman alludes, does more than simply haunt the neurosciences--it demonically possesses it!

D. DO LAWS OF PERCEPTION EXIST?

The adaptability of the human visual system is so great that it is almost as if a universal rule is operative that prohibits universal rules. We might call this *The Rule of Multiple Rules: Slight changes in procedure, stimulus material, or methodology often produce dramatic changes in the rules of perception.*[3] One implication of this generalization is that the perceptual system must now be thought of as operating in a highly active way on any stimulus input rather than in a highly passive and automatic manner. Such a property, along with complexity and multivariateness per se, makes prediction of experimental outcomes and generalization to other experimental situations extremely difficult.

That this generalization should emerge after a century or more of experimental work based on the hope and premise of unification and simplification is unfortunately perplexing, counterintuitive, somewhat distressing, and certainly surprising. It has been virtually axiomatic in psychophysical research that, if we are diligent in our collection of descriptions of the phenomena of vision, in the long run general principles of perception should emerge that will unify the outcomes of what often seems to be, at best, a random assortment of the results of small scale and isolated experiments.

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[2]From one point of view, illusions may be thought of as making the opposite arguments. In spite of repeated demonstrations that a line is straight, or two objects are the same size, or a spiral is standing still, we still "see" these discrepancies with physical reality. However, I believe that this reflects the prepotency of the implied meaning of the stimulus over these other objective measures. Both meaning and secondary measures are examples of cognitive penetration; one merely dominates the other.

[3]Some of the material in the following discussion of the Rule of Multiple Rules has been revised and edited from another of my works (Uttal, 1987), but is considered to be so germane to the current discussion that I have included it here in an edited and updated form.

As successful as the proposition of ultimate generalization has been in other scientific enterprises, *the analogous hypothesis that psychophysical phenomena can also be so unified remains unproven and, astonishingly, largely untested.* Perceptual psychology currently remains a collection of small and seemingly unrelated empirical thrusts. Certainly, it is only in the rarest cases that psychophysical data obtained from different paradigms and under different conditions have even been compared. Perceptual psychophysics has long been characterized by experiments specific to a microscopically oriented theory and by theories that either deal with a narrowly defined data set at one extreme or, to the contrary, a global breadth that is so great that data are virtually irrelevant to their construction. Theories of this kind are more points of view than analyses.

The question posed now is--Is the lack of unification and the absence of truly comprehensive theoretical simplifications (i.e., generalizations), which is apparent in contemporary psychophysical science, a result of the youth of the science or, to the contrary, does it reflect in some fundamental way the actual biological reality of perceptual processes? Though the latter alternative is anathema to both experimental and theoretical psychologists and, from some points of view, a depressing prospect, it can not be rejected out of hand. It is at least conceivable that the perceptual brain-mind operates by means of subprocesses that are more independent and noninteracting than we had either anticipated or hoped. It is entirely possible that superficially similar visual processes may be mediated by quite different underlying mechanisms. There have been so few instances in which a sufficiently wide range of experimental conditions has been explored within the context of a single paradigm that there is actually little support for the antithesis--the idea that unification is, in fact, possible (regardless of how much such an outcome would have pleased us or satisfied William of Ockam or Lloyd Morgan).

A closely related idea is that all perception is entirely uncodifiable and an enormously adaptive inference; a necessary consequence is that because the sensory channels are so heavily coded, the observer can never know the world with certainty. Perception, after all, exists for the survival of the organism, not for the convenience of the theorist. The concept of rigid laws or rules may be another one of those inapplicable ideas uncritically transposed from physics to psychology.

Furthermore, it must not be overlooked that there is also a possibility that the difficulty in identifying universal laws is also caused by differences in the perceptual strategies of individual observers. Evidence that individual differences are larger than we had thought even in carefully controlled and contrived experimental situations can be found in the work of Ward (1985) at even such a relatively early level of processing as that of defining whether stimulus dimensions will be integral or separable in Shepard's (1964) and Garner's (1974) sense. Intraobserver experimental designs (i.e., using the same observer, to the extent possible, for all conditions of an experiment that are to be compared) are absolutely necessary, if it turns out that, in fact, different rules are applied by different observers. Even such designs are not foolproof, however. Shifting relationships among stimuli and alternatively selected responses may result in strategies that vary from task to task even for the same observer.

The evidence for independence of processes, however strongly they may interact, rather than generality, is prevalent in the findings of perceptual psychophysics once one begins to look for it. As one goes from one laboratory to another, or from one research problem to another, there is rarely any linkage between the various outcomes. Furthermore, as we survey the history of psychophysical research, how very often we notice that the classic summary statements are clusters of almost independent rules (e.g., Korte's (1915) laws of apparent movement; Wertheimer's (1923) enunciation of the Gestalt rules of grouping; Grassman's (1854) laws of color mixture; etc.) rather than a single unified conclusion or formula tying together the separate results of experiments carried out in different settings.

Many other psychologists have also noted the absence of universal principles in perception. Hurvich, Jameson, and Krantz (1965) have suggested that this is the case in their insightful comment: "The reader familiar with the visual literature knows that this is an area of many laws and little order."

Ramachandran (1985) phrased it neatly when he raised the possibility that vision is characterized more as "a perceptual bag of tricks" than by great universal principles. Of course, in any theoretical endeavor everything looks like a "bag of tricks" early in the game before the unifying principles become evident. Nevertheless, there is an increasingly large number of observers of this field who agree with the conjecture that a widely diverse set of mathematical models may be necessary to describe what are best viewed as a set of nearly independent visual processes. Grossberg (1983) makes the same point by listing numerous different mathematical models that are now used to describe visual processes of which none seems to be applicable to another. He also alludes to a comment by Sperling (1981) concerning the necessity of multiple formal models (and thus multiple, and presumably independent, internal mechanisms).

A specific instance in which this same sort of idiosyncratic perceptual behavior is rampant has been noted by Grossberg and Mingolla (1985). Pointing out that the way in which texture segregation occurs depends more on the "emergent perceptual units" than on the "local features" of the stimulus, they warn that this "raises the possibility of scientific chaos." In their words:

> If every scene can define its own context-sensitive units, then perhaps object perception can only be described in terms of an unwieldy taxonomy of scenes and their unique perceptual units. One of the great accomplishments of the Gestaltists was to suggest a short list of rules for perceptual grouping that helped to organize many interesting examples. As is often the case in pioneering work, the rules were neither always obeyed nor exhaustive. No justification for the rules was given other than their evident plausibility. More seriously for practical applications, no effective computational algorithms were given to instantiate the rules.
>
> (p. 142)

It should not go unmentioned, however, that Grossberg and Mingolla provide in this article what they believe to be a step forward from the "scientific chaos" that they perceive as such a danger. Their model is based upon a set of analytic expressions that are collectively called the "Boundary Contour System Equations." In their 1985 paper, Grossberg and Mingolla do apply the model to a number of more or less well known perceptual phenomena with a substantial amount of success. These phenomena include certain textural discriminations (Beck, Prazdny, and Rosenfeld, 1983); the neon spreading illusion (Van Tuijl, 1975); the Glass Moire patterns (Glass and Switkes, 1976); and the Cafe Wall illusion (Gregory and Heard, 1979). In doing so, they have linked several visual phenomena to a common mechanism and may have taken a step forward from the "scientific chaos" they have viewed with such alarm.

However, Grossberg and Mingolla do stray from their goal of finding universal mechanisms in a way that suggests that they are still suffering along with the rest of us with the problem of idiosyncratic rules. In analyzing Beck's data, they point to a "remarkable aspect" of perceptual grouping due to colinearity. They ask: "Why do we continue to see a series of short lines if long lines are the emergent feature that control perceptual grouping?" (p. 150). Their response to this question is to invoke at least two separate and distinct perceptual "outputs" from the boundary-contrast system, one of which is terminator sensitive and one of which is not. Both, however, influence the perceptual outcome of the stimulus. Unfortunately, this invocation of multiple mechanisms appears to me to be conceptually identical to the "idiosyncratic rules" solution to the scientific chaos problems in visual psychology about which they and others have complained. The invocation of multiple mechanisms is identical, in principle, to permitting additional degrees of freedom in an increasingly flexible model within which a wider variety of functions can be fit.

The situation seems the same even when we are dealing with as specific and fundamental a problem as the search for a putative universal metric of visual space. How does the visual system distort or transform physical space as it views it with its "cyclopean eye"--an "eye" influenced by many monocular and dichoptic cues. Wagner (1985), in the very act of presenting a new metric for the transformations assayed by his experimental procedure, came to the conclusion:

> In sum, this multiplicity of well-supported theories indicates that no single geometry can adequately describe visual space under all conditions. Instead the geometry of visual space itself appears to be a function of stimulus conditions.
>
> (p. 493)

and, I might add, of procedure as well.

Haig (1985) alludes to the same limitations on the search for generalities with regard to face recognition when he notes:

> Individual differences (in recognition strategy) are strong, however, and the variations are such that the uncritical application of generalized feature salience lists is neither useful nor appropriate.
>
> (p. 601)

Haig also goes on to explain that different stimulus faces seem to evoke different recognition strategies, thus further complicating the search for simple rules of face perception in particular and form perception in general.

It is possible that we simply do not yet perceive the grand scheme because our experiments have been too spotty and disorganized. The unfortunate conclusion is that whatever the youth of this science, the current state of theory is one that seems to support the unhappy conclusion that separation and independence of the constituent processes of perception and idiosyncratic behavior may be real and not artifacts of an inadequate experimental technology.

It should also be noted, lest one incorrectly concludes that the absence of general rules is unique to vision, that the underlying separateness of function seems also to be typical of many other cognitive processes. Indeed, in a recent report, Hammond, Hamm, and Grassia (1986) summed up the general problem in the following way:

> Doubts about the generality of results produced by psychological research have been expressed with increasing frequency since Koch (1959) observed, after a monumental review of scientific psychology in 1959, that there is "a stubborn refusal of psychological findings to yield to empirical generalization" (pp. 729-788). Brunswik (1952, 1956), Campbell and Stanley (1966), Cronbach (1975), Epstein (1979, 1980), Einhorn and Hogarth (1981), Greenwald (1975, 1976), Hammond (1966), Meehl (1978) and Simon (1979) among others, have also called attention to this situation. Jenkins (1974), warned that "a whole theory of an experiment can be elaborated without contributing in an important way to the science because the situation is artificial and *nonrepresentative*" [italics added] (p. 794). Tulving (1979) makes the startling observation that "after one hundred years of laboratory-based study of memory, we still do not seem to possess any concepts that the majority of workers would consider necessary or important."
>
> (p. 3)

Hammond, Hamm, and Grassia (1986) argue that, at least in the fields that they have surveyed, this situation is caused not by the nature of human biology, but, rather, by the absence of an appropriate analytic methodology. They propose a technique they suggest would help to alleviate the lack of generality in studies of cognitive judgment--their field of interest. It is not possible for me to judge if their technique is suitable for the kind of perceptual separateness observed in the perceptual domain--the reader will have to refer to their work for details; here I merely raise the issue for other students of cognitive psychology to ponder.

The list of other distinguished psychologists who have made the same point includes Ulrich Neisser (1976). He also noted the absence of generality and of the limits of psychological facts to the specific experiments that originally elucidated them in the field of cognitive psychology.

In summary, a considerable body of theoretical and empirical research, therefore, does seem to currently support the argument that the perceptual system is a constellation of relatively idiosyncratic and independent information processing engines. Furthermore, analyses of a variety of higher level cognitive approaches also suggest that narrowness, specificity, and a lack of generality characterize work in that domain.

We should make no mistake about this point; however abstract and esoteric it may seem, however remotely "philosophical," the issue raised is fundamental. Have we missed the generalities (assuming they are there in some true biological sense) because of the method of detail that we must use for practical, paradigmatic reasons? Or, to the contrary, has our "hope" that these generalities exist blinded us to a very important, although contradictory, generality in its own right--namely, that because of the enormous adaptability of the human cognitive system (i.e., the mind), there are few perceptual generalities beyond the most global or the most trivial to be discovered concerning visual perception?

To conclude this discussion, it may be more positively hypothesized that perhaps the elusive laws and aggravating variability observed in human form perception are but other arguments for the deep cognitive penetration of our visual processes. Perhaps we will have to accommodate ourselves to the performance of a system that itself is so adaptive that it permits strategy shifts in what are ostensibly the most well controlled experiments, and that allows wide-ranging individual differences to influence data. That system may operate, in general, more by what has classically been called *rationalistic* than by *empiricistic* principles.

E. A SUMMARY AND A PRESCRIPTION FOR THE FUTURE

In summary, my review of the data and theories of visual form perception in the preparation of this volume has left me with two very different views of the nature of psychobiological reality in this science--what the science is at present, and what it should be in the future. Clearly we are in a phase of the study of form perception that is characterized by the terms elementalistic, empiricistic, and reductionistic.

Elementalistic Form Perception

There is a pervasive local feature orientation in modern theory, perhaps due to the absence of good tools to study overall organization. This is in contrast to the demonstrations and formal data that seem to support a holistic, Gestalt, global kind of thinking emphasizing the arrangement of the parts rather than the nature of the parts.

Empiricistic Form Perception

The vast amount of contemporary theory in form perception assumes that the perceiving organism operates on the basis of passive, automatic, algorithmic interpretations of those local features by what are essentially rigid and mechanical computational engines. This is in contrast to a vast amount of phenomenal evidence that form perception is so adaptive and interpretive

and is so influenced by the meaning of the stimulus, that it would be better to classify it as rationalistic.

Reductionistic Form Perception

There is an enormous contemporary confidence that form recognition can be analyzed into the underlying constituent mechanisms and processes in the not-too-distant future. This philosophy operates at two levels. First, it is assumed that neurophysiological findings can be used as a model of form perception in spite of the arguments from chaos, automata, and combinatorial considerations that such a reductionism is, in principle, not to be realized. Second, it is also assumed that perception can be reduced to units of cognitive process, which themselves may or may not be reducible to neurophysiological terms. Such a conviction, however, also flies in the face of the arguments that "black boxes" cannot have their internal mechanisms uniquely defined by input-output methods alone and that, for combinatorial reasons, neither can the cellularly oriented neurophysiologist-neurosurgeon open the "black box."

What major guidelines should direct perceptual research in the immediate and long-term future? Here are my suggestions.

1. First, it must be recognized that the many applied computer vision and artificial intelligence models of human form perception, however useful they may be and however well they mimic the properties of human vision, are not necessarily valid explanations or theories of human vision. Indeed, it can be argued that some models (such as the "expert system") are a complete surrender of the hope that we can really model human mental processes. These table-lookup operations clearly do not model the way human associative thinking works, but simulate the behavior by logics and mechanisms that are beyond a doubt entirely different than those used in human cognition. To put it bluntly, "expert systems" may be the unacknowledged swan song of a dying belief--that natural intelligence can be realistically modeled on computers. It is essential that the relationship between an imitation by an analogy and the elucidation of a homologous logical mechanism must be clarified and understood. Even though we can admire and respect the practical and useful accomplishments of this field of engineering application and development (i.e., AI), we must rid ourselves of any misconception that such engineering tools are any more likely than any other type of model to be valid theories of human perception.

Computational modeling of visual processes represents a special case and is an especially seductive quasi-theory, but it must also fall victim to this same criticism in the final analysis. Computational models are designed to simulate the transformations that *must* be executed to go from the informational state defined by the stimulus input to the state described by the perceptual phenomenology. As such, they are also process analogs and may plausibly invoke any useful (and available) mathematical or computational process to accomplish the transformation. Although this is an extremely useful approach to understanding some of the transforming steps, it does nothing to tell us which of the many possible alternative mechanisms or logics within the

visual system is the one that actually carries out these transformations. Indeed, the steps need not even be computationally defined in the sense demanded by Marr and his collaborators. The human brain may use approximate processes quite unlike the Marrian algorithms. These approximations may depend upon linkages of meaning and global organization rather than upon numerical or algebraic transformations of local attributes. These processes (or combinations of processes) may produce solutions to perceptual tasks that are totally adequate, even though not satisfying the criteria of the computational theorists as a good model. In short, computational modeling is also subject to the limits imposed by the Moore theorem and the fundamental indeterminacy of mathematical descriptions or input-output analysis with regard to internal structure.

2. My second proposed guideline requires acknowledgment that human visual perception is mainly holistic in its operation. The Gestalt psychologists understood and correctly taught this principle, but their wisdom was not influential because the computational and mathematical technology that was needed to pursue the holistic strategy was not then and is not now available, for all practical purposes. We have an enormous obligation to convince mathematicians to develop techniques better suited to studying arrangement than parts. A major effort is necessary to develop the appropriate mathematics in order that some future equivalent of a noneuclidean mathematics can be utilized by some future equivalent of an Einstein to make the much needed breakthrough in visual theory, so that our science can enjoy the same kind of growth in understanding. This breakthrough may not be in the form of general principles, but perhaps a softer kind of mathematics able to handle different kinds of relationships beyond added to, subtracted from, or multiplied and divided by.

3. Next we must determine what limits apply to the goals of visual theory building. There is an urgent need for additional efforts to determine what constraints are operating on this science so that we can avoid a naive and enormous waste of impossible theory-building energy. That there should be limits is in no sense a condemnation of perceptual psychologists or psychology any more than acceptance of the limits on perpetual motion or speed of light are of physics. It is clear, however, that perceptual science can be correctly and justifiably criticized for not paying sufficient attention to the fundamentals before going off ill-prepared and overconfident into the heady world of neurophysiological or cognitive process reductionism. I am convinced that a few more skeptical combinatorial, automata, or chaos theorists interested in the problems raised by perceptual psychology would do more for the future progress of our science than an army of "true believers" in the ultimate solvability of all our problems.

4. Another guideline stresses that the empirical psychophysical approach, in which the phenomena are sought, discovered, and described, must be the centerpiece of any new development in this science. This empirical effort, however, should be redirected to emphasize the global or holistic properties of stimuli rather than the local ones currently in vogue. This is the most effective means of diverting the Zeitgeist from what it is to what it should be.

5. Further, we must develop mathematical models that concentrate on quantifying, formalizing, and describing reported perceptual phenomena. But not just any models--it is mandatory that there be a conscious effort to develop techniques that emphasize the global and organizational attributes of a stimulus-form. It is also mandatory that we understand the intrinsically nonreductive nature of mathematics in this kind of theory building.

6. Reluctantly, given my personal scientific background, I think that we are going to have to abandon the idea that perceptual processes can be reduced to neurophysiological terms. This romantic notion, this will-of-the-wisp, this dream, is almost certainly unobtainable in principle as well as in practice if combinatorial and chaos theory do turn out to be applicable to this domain of inquiry. What we know about the metabolism and physiological functioning of individual neurons, though a distinguished intellectual and scientific accomplishment in its own right, can probably never be transformed into knowledge of how they operate collectively in the enormous networks of the brain to produce molar behavior.

7. Equally reluctantly, I believe an appreciation must emerge that the major goal of cognitive psychology--to determine the functional processes that are carried out by the nervous system in form perception or, for that matter, in any other kind of mental activity--will always be equivocal. Not only have the data been inconsistent, but so too have been the conclusions drawn. These outcomes reflect the enormous adaptability of the perceiver on the one hand, and the fundamental indeterminateness of any theory of the processes going on within what is, for any conceivable future, a closed system.

8. We will also have to come to appreciate that the study of perception, as all of the other cognitive processes, is an information processing science and not an energy- or matter-processing one. The nature of internal codes and representations, therefore, is far more arbitrary and complex, and the laws describing operations are necessarily going to appear to be far less rigid than those emerging from the study of simple physical systems. Indeed, there is even a question whether or not the general concept that "laws" exist that are operative in the energy/matter dominated fields of science may be transferable to this much more multivariate domain of perceptual processes. I argue that stimuli do not lead inexorably to responses by simple switching, circuit-like behavior. Rather, it seems that there is a rational, meaningful, adaptive, utilitarian, and active construction of percepts and responses by mechanisms that depend more upon the meaning of a message than its temporal or spatial geometry. In the perceptual world, information, unlike matter or energy in the physical world, can actually be created and destroyed.

9. We are going to have to accept the reality of mental processes, and the fact that these processes are the result of ultra-complex neurophysiological mechanisms. The basic principle of psychobiological monism asserts that there is nothing supernatural, extranatural, or even separate at work in mental activity--it is nothing more or less than one of the processes of neural activity. But we do have to appreciate that complexity and numerosity themselves can exert influences that come perilously close to producing exactly the same kind of mysteries that would appear if there were unnatural forces at work. Thus, we must at once reaffirm our commitment to psychobiological monism (without which any scientific study of the mind would certainly

perish) and at the same time acknowledge that the gap between the two levels of discourse--neuronal network state and mental phenomenon--may never be crossed. This requires an epistemological or methodological behaviorism in practice and a metaphysical neuroreductionism in principle. The intrapersonal privacy of perception may also require some compromises in that it, too, forces us to simultaneously function as behaviorists and introspective mentalists. Nevertheless, we must accept the facts that mind, in general, and perceptual experience, in particular, exist, that they are unobservable directly, and that they can at best only be inferred from behavioral responses by observers. The interpersonal privacy of a percept may be as much a barrier to analysis as is the combinatorial limit or the "black box" constraint.

10. Finally, we are going to have to accept the primacy of the phenomena in any controversy between different points of view or theories in perceptual science. That is, the final arbiter of any explanatory disagreement or controversy must be the reported nature of the perceptual experience. Neurophysiology, mathematics, parsimony, and even some kind of simplistic plausibility are all secondary and incomplete criteria for resolving such disputes. The perceptual experience is the final outcome of a concatenation of processes and is complete in the sense that it reflects all of the relevant previous steps. Anything--idea, theory, formal model, or verbal explanation--that is in conflict with the perceptual phenomenon, in principle, requires modification or rejection. This does not mean that the percept can define everything or even indicate to us what the underlying processing steps were, but rather that in those cases where a conflict between observation and explanation does occur, the former must be definitive. At a qualitative level the perceptual phenomenon is also a good source of heuristics for theory building in this science simply because it is the stuff of this science. Perceptual psychologists are primarily in business to describe and explain the psychobiological reality we call perceptual experience, not to exercise computers or to speculate about uses for the increasingly large number of anatomically or physiologically specialized neurons that are appearing at the tips of our microelectrodes.

In short, what I am proposing is a *mathematically descriptive, nonreductionistic, holistic, rationalistic, mentalistic, neobehaviorism* that is guided more by the relevant phenomena than by available analytic tools--a neobehaviorism ambitious to solve the classic problems of perceptual psychology, but modest in avoiding recourse to strategies that are patently beyond the limits of this or any other science. All too much of our effort has been spent on unattainable goals in the past few decades. I believe such a strategy would be a step toward a realistic and mature scientific approach to how people see forms.

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